

January 13, 1962

Dear George--

I do not know how much interest these tidbits of news will have for you. I was sorry to see how much headway the cancer quackery had made, but at least it did stir up a respectable scientific and medical reaction, at least in so far as it was reported in the N.Y. Times.

The Kennedy Foundation affairs, and the connected panel on mental retardation have been taking a tremendous amount of time, and together with other matters have virtually kept me from the laboratory. But happily we now have a quite definite commitment from the foundation for \$1,000,000 towards the "laboratories for molecular medicine-- dedicated to the study of mental retardation -- molecular biology, heredity, neurobiology, developmental medicine". As this can be matched by an equal amount of other funds, and then again by NIH building funds, this enables us to go ahead with definite planning for our "Blank Building" to be finished in some 2.5 years from now. All this has generated a definite interest to do something about the molecular biology of the central nervous system in relation to intellectual development; apart from Hyden's work, which has not gotten down to many specifics, this is mostly terra incognita. When we have the new space, and perhaps sooner, we propose adding a laboratory for protein chemistry, mainly devoted to the CNS, but also to help our other work in microbial genetics, etc. The immediate need, it seems to me, is simply to characterize the proteins of the CNS, and from this to hope to have some clues as to their functional significance. (There is already a report of a polymorphism of C57 vs. CBA mice, as you can see from the enclosure; someday, someone should make these coisogenic so it will make some sense to make behavioral comparisons.) If you can think of anyone who could be qualified to work in this area, we should have a very attractive position for him.

One related question has come up -- how to study the basis of the age-dependence of oocyte nondisjunction leading to mongolism. Of course this could be a matter of fetal viability; it might also relate to steroid changes with aging that influence the integrity of the egg and its meiotic spindle, or chromosome exchange. We should have a model system in an experimental animal, and it occurred to me that it would be interesting to see how long one could retransplant ovaries in mice to young animals of the same strain, and maintain the function of the old ovaries. Has anyone done such an experiment? (It is of course the converse of the hypothetical situation if the hormonal environment is the critical factor.) It should be a simple addition to your present program if you would be curious enough just to see what happened.

Is Moller coming to Gatlinburg as planned? If so should we sustain our invitation for him to visit here, either just for the occasion, or for some possibility of a longer visit? I hope my bringing this up doesn't disturb you, but there should be a definite answer on it, and I must leave it entirely to you to communicate with him on this according to your own best judgment. We are leaving for 6 weeks in Japan on April 7 or 10, and his itinerary should therefore be made for before then if he is to visit while we are still here. (Travelling is still the bane of my existence

--mostly to Washington, but in a couple of weeks to Mexico, which may be more congenial). }

Some exciting news from Burnet, partly via Gus; perhaps you have already heard of it : the injection of testosterone into young chick embryos suppresses the subsequent development of the "Bursa of Fabricius" and in some cases the thymus, giving something analogous to agammaglobulinemia; but the birds still reject homografts. It should be obvious to do something of the same sort with mice. You mentioned some rather puzzling experiments on the permanent castration of female mice by early administration of estrogen -- do I remember correctly? What was the literature reference to that? (If there are any specific steroids you could have any use for, in this or any other connection, please let me try to see whether Syntex has them, and of course they would be freely available).

With subtilis DNA, the digression experiments have all collapsed for one technical reason or another, and are not being pursued now. No new linkages have been found; Gene is writing up what he has on the his/tryp linkage group. Most exciting remain the separation studies by Gan, on fractionation of different genes in CsCl gradients; even better by differential melting ~~point~~ followed by the removal of denatured fractions of the DNA. There is some more explicit evidence (spectroscopic) that the different fractions, enriched for different genes also vary in actual composition, the GC/GC + AT ratio varying from 38 to 41 %. Walter Bodmer is also having great fun on the differential sensitivity of markers and linkage groups to different nucleases. Lou Baron is here for 6 months, starting to work on subtilis phages and dna, but finding this overlapped a great deal with Romig at UCLA switched to some studies of a lysozyme- (and autolysis-) resistant mutant which has a remarkable array of pleiotropic effects.

Be well; please let me hear from you when this is possible.

Enc: some trivialities and amusements